Journal of Abnormal Psychology
2000, Vol. 109, No. 4, 624-633

In the public domain
DOI: 10.1037//0021-843X.109.4.633

Posttraumatic Stress Disorder and Retrospectively Reported Stressor Exposure: A Longitudinal Prediction Model

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There has been recent concern about the degree to which posttraumatic stress disorder (PTSD) symptomatology influences reports of prior exposure to highly stressful life events. In this longitudinal study of 2,942 male and female Gulf War veterans, the authors documented change in stressor reporting across 2 occasions and the association between change and PTSD symptom severity. A regression-based cross-lagged analysis was used to examine the relationship between PTSD symptom severity and later reported stressor exposure. Shifts in reporting over time were modestly associated with PTSD symptom severity. The cross-lagged analysis revealed a marginal association between Time 1 PTSD symptom severity and Time 2 reported stressor exposure for men and suggested that later reports of stressor exposure are primarily accounted for by earlier reports and less so by earlier PTSD symptomatology.

Posttraumatic stress disorder (PTSD) is an anxiety disorder in persons exposed to an extremely stressful event that evokes feelings of intense fear, helplessness, or horror (American Psychiatric Association, 1994). The condition encompasses symptoms of intrusive thoughts and reexperiencing of the event, emotional numbing and avoidance of event reminders, and hyperarousal. Not only is the event required for a PTSD diagnosis but a good deal of the symptoms (e.g., intrusive thoughts, avoidance of reminders) revolve around a replay of the event. The veracity of the report of the event is critical for diagnostic purposes as well as for etiological research on the causal impact of stressors on psychological functioning.

Research studies that solicit retrospective accounts of events and circumstances to predict current psychosocial status—indeed, almost all PTSD studies—present threats to the internal validity of causal inference (Cook & Campbell, 1979; D. W. King & King, 1991). Retrospective data are subject to problems in simple recall and vulnerable to reconstruction of details that vary in objectivity and accuracy. They may be influenced by research participants' reporting biases, such as those arising from a social desirability or other response style, demand characteristics within the research setting, or investigator expectancies. Moreover, as discussed by Nisbett and Wilson (1977), Metts, Sprecher, and Cupach (1991), and Brewin, Andrews, and Gotlib (1993), one's memory of a past experience, the description of its features and nuances, and the meaning assigned to it are necessarily influenced by one's state of mind.

Such problems with retrospective data are particularly salient in the study of stress and trauma. B. P. Dohrenwend and his associates (e.g., B. S. Dohrenwend, Dohrenwend, Dodson, & Shrout, 1984; B. P. Dohrenwend & Shrout, 1985) argued that an understanding of the stress process requires a differentiation between "pure environmental events, uncontaminated by perceptions, appraisals, and reactions" (B. P. Dohrenwend & Shrout, 1985, p. 782) and the characteristics of the individual and setting within which the events occur. In other words, an event or stressor is best considered an unequivocally objective phenomenon, with little or no subjective aspects. Yet this conceptualization is a challenge to stress and trauma field researchers, who have minimal ability to control or anticipate highly stressful experiences that might befall individuals. Concomitantly, rarely is there a truly objective measure of the severity of the event at the time of its occurrence. Researchers, therefore, are left to rely on postevent recollections of the experience.

The preponderance of PTSD research has used cross-sectional designs (with retrospective self-reports of prior events and circum-

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This research was supported by a Department of Veterans Affairs Medical Research Service Award and by assistance from the Department of Veterans Affairs Mental Health Strategic Healthcare Group (Paul Errera and Thomas Horvath). We thank Bruce Dohrenwend for stimulating our pursuit of this research topic. We also express our appreciation to Charity Hammond for assistance in data analysis.

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stances). These designs, of course, preclude an unambiguous assertion of cause-and-effect and offer little resolution of issues related to subjectivity in reporting or shifting perceptions of a prior experience as a function of psychopathology. Longitudinal designs, with repeated measures of both the stressor and PTSD over occasions, are better suited to clarifying temporal precedence and thus directionality.

To our knowledge, only three longitudinal studies have attempted to address empirically the directionality of the stressor–PTSD relationship. Roemer, Litz, Orsillo, Ehlich, and Friedman (1998) recently documented consistent increases in reports of exposure to seven specific war-related stressors over time in a sample of 460 soldiers deployed to the Somalia peacekeeping operation. The first assessment was in the first year after return, and the second assessment was 1 to 3 years later, with an average interval of 21 months. These researchers then used a hierarchical multiple regression approach to predict Time 2 reported exposure, controlling for Time 1 reported exposure. They found that Time 2 PTSD symptomatology, particularly the symptom cluster reflecting intrusive thoughts, uniquely contributed to Time 2 exposure scores. They interpreted their findings as evidence that PTSD symptomatology can influence reports of prior stressful events or circumstances, yet the direction remains ambiguous because the significant stressor–PTSD association was between measures administered at the same second assessment.

The other two studies that addressed the directionality issue used change scores and bivariate correlations. Wyshak (1994) documented change in event reporting over a 1-week interval for a sample of 29 Southeast Asian refugees. She found that change was significantly and inversely associated with Time 1 PTSD symptomatology ($r = -.48$). Those who most changed their endorsements of experiencing, witnessing, or hearing about incidents of trauma and torture tended to have lower self-reported PTSD symptom severity scores. As Wyshak herself noted, the sample was quite small and the interval between assessments was brief. More important to the purpose of the present study, she did not differentiate the type of change in symptom reporting (shifts from endorsement to nonendorsement vs. shifts from nonendorsement to endorsement). Hence, the role of psychological status in motivating increased endorsement of prior stressful life events was not examined.

Finally, Southwick and his associates (Southwick, Morgan, Nicolaou, & Charney, 1997) administered a 19-item war-zone exposure questionnaire and the Mississippi Scale for Combat-Related PTSD (Keane, Caddell, & Taylor, 1988) to 59 members of the National Guard who had been activated for Gulf War duty. Time 1 assessment was at approximately 1 month after the veterans' return, and Time 2 assessment was about 2 years after return. Southwick et al. found that, on average, the number of endorsed events increased somewhat over testing occasions; that 88% of the veterans changed at least one response; and that more changes were from nonendorsement to endorsement than from endorsement to nonendorsement. Of particular note, the number of no-to-yes changes was significantly and positively related to PTSD symptom severity at Time 2 ($r = .32$). The researchers concluded that psychopathology may be associated with amplification of memory for traumatic events over time.

The purpose of this study was to examine further the association between retrospective self-reports of exposure to highly stressful events and circumstances and PTSD. We built on the Southwick et al. (1997) study in that we also used a sample of Gulf War veterans and focused on war-related stressors and their sequelae, each assessed shortly on return from the Gulf region and again some 1 ½ to 2 years later. We had the benefit of a much larger and more diverse sample of male and female soldiers. Furthermore, we used cross-lagged analysis of a longitudinal panel design, a methodology intended to elucidate the issue of directionality of cause-and-effect and more straightforwardly answer the question: To what extent does PTSD symptom severity alter later reports of stressor exposure?

Method

Participants

A total of 2,942 individuals participated in the study: 2,702 male (92%) and 240 female (8%) U.S. Army soldiers who were deployed to the Gulf region from Fort Devens, Massachusetts. Their mean age was 30.16 years ($SD = 8.42$); men averaged 30.30 years ($SD = 8.50$), and women averaged 28.10 years ($SD = 7.06$). The mean amount of education was 13.17 years ($SD = 1.80$). Approximately 57% reported that they were currently married, 35% were single and never married, and the remaining 8% were separated, divorced, or widowed. The sample was predominantly Caucasian (83%), with 9% identifying as African American, and 4% identifying as Hispanic or Latino, with the remainder indicating they were from other racial or ethnic groups. About 8% of these soldiers were commissioned officers; the majority were either in the enlisted (38%) or noncommissioned officer (55%) ranks. The sample was composed of 28% active duty personnel, 52% called to duty from the National Guard, and 20% activated military Reservists. Only 12% reported having previous combat experience. Of this full sample of individuals assessed at Time 1, 78% supplied Time 2 data. A previous analysis of an array of demographic characteristics of the cohort indicated that those who did not provide data on the second occasion were younger and more likely to be minority and active duty soldiers (see Wolfe, Erickson, Sharkansky, King, & King, 1999, for further details).

Measures

Reported war-zone stressor exposure. This was measured by 31 items, including items from the Laufer Combat Exposure Scale (Gallops, Laufer, & Yager, 1981), as well as items intended to represent events and circumstances unique to the Gulf War. Each item was scored in a dichotomous manner: 1 = endorsement or *yes* (self-report of exposure to the stressor); 0 = nonendorsement or *no* (self-report of no exposure to the stressor). Sample items included: "Did your unit engage the enemy in a firefight?" "Did you see Americans or other troops killed or wounded?" "Were you part of an assault on entrenched or fortified positions?" "Were you ever on formal alert for chemical or biological attack?" A total score on this measure was computed as a simple count of all endorsed items.

PTSD symptom severity. This was assessed with Keane et al.'s (1988) Mississippi Scale for Combat-Related PTSD modified for use with Gulf War veterans. This 35-item instrument uses a 5-point Likert response format to measure the reexperiencing, avoidance and numbing, and hyperarousal components of PTSD, along with the associated features of depression, substance abuse, and suicidal tendencies. The Mississippi Scale repeatedly has demonstrated high internal consistency and test–retest reliability, with coefficients typically in the .90s (e.g., Hyer, Davis, Boudewyns, & Woods, 1991; Keane et al., 1988; Kulka et al., 1990; McFall, Smith, Mackay, & Tarver, 1990). It likewise has a sound record for validity in terms of expected associations with combat exposure (e.g., Keane et al., 1988; D. W. King, King, Foy, Keane, & Fairbank, 1999) and other PTSD measures (e.g., Kulka et al., 1990; McFall, Smith, Roszell, Tarver, &

Malas, 1990). For the present study, the internal consistency reliability index was .89 for Mississippi Scale data collected on the first occasion and .93 for data collected on the second occasion.

Procedure

Participants completed questionnaires containing a number of self-report measures, at two time points, within 5 days of their return to the United States and before reuniting with their families, and again 18 to 24 months later. The Time 1 questionnaire contained a section to ascertain demographic information, and both the Time 1 and Time 2 questionnaires contained the measure of war-zone stressor exposure and the Mississippi Scale. All Time 1 data were obtained in face-to-face unit meetings; Time 2 data were collected in face-to-face unit meetings, where feasible, and otherwise by mailed questionnaire or telephone interview when unit meetings were not possible (e.g., the individual was no longer associated with the military or was stationed overseas).

Analyses

Descriptive and bivariate analyses. We computed means, medians, standard deviations, and intercorrelations for the war-zone stressor exposure and Mississippi Scale scores. We then documented changes in reporting of stressor exposure, both in terms of the participants' patterns of responses to each individual stressor item from Time 1 to Time 2 and in terms of the count of the number of changes across items for each study participant. With regard to the latter, we computed three frequency distributions to describe the amount of change in the sample: (a) total number of changes from endorsement to nonendorsement, or yes-to-no shifts; (b) total number of changes from nonendorsement to endorsement, or no-to-yes shifts; and (c) total number of changes of either type. These change indices were correlated with PTSD symptom severity as represented by the Time 1 and Time 2 Mississippi Scale scores.

Cross-lagged panel analysis. For the cross-lagged panel analysis, we used a structural equation modeling approach (Ecob, 1987; Farrell, 1994; Williams & Podsakoff, 1989). We used four latent variables: reported stressor exposure and PTSD symptom severity at the two time points. The exposure latent variable was represented as a single causal indicator (Bollen & Lennox, 1991; Cohen, Cohen, Teresi, Marchi, & Velez, 1990; Loehlin, 1992; MacCallum & Browne, 1993), wherein exposure to events or circumstances was specified to "cause" the experiencing of stress or score on the war-zone stressor exposure measure. At both Time 1 and Time 2, the total number of item endorsements was the indicator of reported stressor exposure. Each of the PTSD symptom severity latent variables (Time 1 and Time 2) had four effect indicators, formed by grouping Mississippi Scale items and computing average item scores. The four Mississippi Scale item clusters or "mini-scales" represented the dimensions of reexperiencing and situational avoidance (11 items; Time 1 α = .82, Time 2 α = .87), withdrawal and numbing (11 items; Time 1 α = .75, Time 2 α = .83), hyperarousal and lack of control (8 items; Time 1 α = .72, Time 2 α = .78), and guilt and suicidality (5 items; Time 1 α = .48, Time 2 α = .60). We previously identified and supported these item clusters in a series of exploratory and confirmatory factor analyses of the Mississippi Scale (L. A. King & King, 1994) and used them in a set of studies of Vietnam veteran functioning (D. W. King et al., 1999). As is customary in the confirmatory factor analysis component of the modeling procedure, each of the four observed scores was regressed on the PTSD factor for Time 1 and Time 2, with the strength of the association between observed score and factor score represented by the factor loadings.

Using these latent variables and their indicators, we specified and tested a multiple group measurement model for men and women; we then tested the structural models, evaluating the cross-lagged relationships between reported stressor exposure and PTSD symptom severity. In all analyses, previous combat experience, indexed as a 0/1 dichotomy, served as a covariate to control for possible war-related experiences prior to deployment to the Gulf War (Muthen, 1989). We assessed the viability of the cross-lagged effects and, where viable, then assessed their equivalence. This was accomplished by specifying a series of nested models, proceeding from the most saturated to the most constrained. The first comparison of interest was between a model in which all regression coefficients were specified as free and a model in which the cross-lagged paths were fixed at zero. If the former model was favored, then a model having equivalent cross-lagged coefficients was specified and evaluated vis-à-vis the most saturated model. The structural model included two multiple regression equations: (a) Time 2 PTSD symptom severity regressed on Time 1 PTSD symptom severity and Time 1 reported stressor exposure; and (b) Time 2 reported stressor exposure regressed on Time 1 reported stressor exposure and Time 1 PTSD symptom severity. Of particular interest were the cross-lagged paths: Time 1 reported stressor exposure to Time 2 PTSD symptom severity, and Time 1 PTSD symptom severity to Time 2 reported stressor exposure.

The Mplus software package (Muthen & Muthen, 1998) was used for the structural equation modeling. This program has the advantage of accommodating incomplete data, even though missingness may not be fully random. According to Graham, Hofer, Donaldson, MacKinnon, and Schafer (1997) and Little and Rubin (1987), among others, there are three conditions under which data may be missing: (a) completely at random or by the design of the researcher; (b) at random, where missingness is related to one or more known variables in the data; and (c) not ignorable, in which missingness is related to variables that are neither known nor present in the data. Maximum likelihood estimation procedures, such as the one used in the Mplus program (Muthen & Muthen, 1998, Appendix 6, pp. 287–288), are considered acceptable for the first two conditions (Graham et al., 1997; Little & Rubin, 1987). In our case, incomplete data were best judged as missing at random, the second condition. Moreover, covariance coverage values, reflecting the proportion of data present, ranged from 76% to 100%, the former figure well above the recommended 10% minimum. As a consequence, data from all those who participated in the Time 1 assessment were available for analysis.

Results

Descriptive and Bivariate Analyses

Table 1 presents means, medians, standard deviations, and intercorrelations among the four measured variables. As shown, average scores on both the reported stressor exposure measure and the Mississippi Scale increased somewhat over the interval between administrations. As might be expected, the stability coefficients, the correlations between the same measures across time, were the highest. The synchronous correlations between different measures administered on the same occasion were substantially lower and quite similar to the cross-lagged correlations between different measures on different occasions.

Table 2 itemizes the distributions of the patterns of responses to each reported stressor exposure item over the two occasions. The amount of change of either type (endorsement to nonendorsement, or nonendorsement to endorsement) ranged from virtually 0% (amphibious invasion, battle fatigue, and prisoner of war) to approximately 30% (training accidents and forward observation post or base camp). The mean change across all items was 10%. With three exceptions (receive incoming fire, engage in a battle with considerable casualties, and amphibious invasion), the number of no-to-yes changes exceeded the number of yes-to-no changes.

The frequency distributions characterizing the number of changes in stressor endorsements for the sample are presented

Table 1
Descriptive Statistics and Intercorrelations Among Time 1 and Time 2 Measures

Measure	N	Descriptive statistics			Intercorrelations			
		M	*Mdn*	*SD*	1	2	3	4
1. Total reported stressor exposure, Time 1	2,756	4.51	4.00	3.01	—			
2. Total reported stressor exposure, Time 2	2,123	5.40	5.00	3.57	.73*	—		
3. Total Mississippi Scale score, Time 1	2,777	60.61	58.33	13.11	.25*	.25*	—	
4. Total Mississippi Scale score, Time 2	2,249	65.68	61.77	17.41	.25*	.36*	.61*	—

Note. Correlations are computed as pairwise relationships, $N = 2{,}001$–$2{,}777$. Similar tables for men and women separately are available from Daniel W. King.
* $p < .001$.

in Table 3. The rightmost columns, those pertaining to the frequency of change of either type, show that only about 9% of the sample responded in a totally consistent manner on both occasions. Approximately 94% of the sample made 6 or fewer changes of either type. About 90% made 4 or fewer no-to-yes changes, and just under 90% made 2 or fewer yes-to-no changes. On average, the number of changes of either type was 3.04 ($SD = 2.05$), the average number of no-to-yes changes

Table 2
Distributions of Response Patterns for Stressor Items

Item	Responses (%)				
	No at both times	Yes at both times	Yes at Time 1, No at Time 2	No at Time 1, Yes at Time 2	Change
. . . unit suffer training accidents?	43.7	25.0	12.7	18.6	31.3
. . . stationed at a forward observation post or base camp (i.e., close to enemy lines)?	41.6	30.1	9.0	19.3	28.3
. . . see . . . troops killed or wounded?	43.9	30.8	7.6	17.7	25.3
. . . experience equipment failures that jeopardized the safety . . . of yourself or your buddies?	66.2	10.7	7.8	15.3	23.1
. . . communications ever cut off between your unit and other units?	63.8	13.8	10.0	12.4	22.4
. . . on formal alert for chemical or biological attack?	19.9	60.2	8.9	11.0	19.9
. . . see civilians . . . killed or disfigured?	59.2	21.5	8.1	11.2	19.3
. . . see enemy troops . . . killed or disfigured?	42.1	39.7	6.5	11.8	18.2
. . . receive . . . incoming fire from artillery, SCUD rockets, mortars or bombs?	26.9	57.0	8.7	7.4	16.1
. . . encounter mines or booby traps . . .?	63.9	22.2	3.9	10.0	14.0
. . . receive . . . incoming fire from small arms?	80.0	6.4	3.5	10.1	13.6
. . . on ship or aircraft . . . that passed through hostile waters or airspace?	80.9	6.0	2.8	10.2	13.1
. . . surrounded by the enemy?	90.3	1.7	2.5	5.5	8.1
. . . sit with anyone dying from battle wounds?	88.7	3.5	2.4	5.4	7.8
. . . unit receive sniper or sapper fire?	88.8	3.5	2.1	5.6	7.7
. . . attacked by terrorists or civilians?	92.9	1.1	1.9	4.1	6.0
. . . part of an assault on entrenched or fortified positions?	92.5	1.6	2.1	3.8	5.9
. . . make or receive any . . . decisions that . . . resulted in death or serious injury to members of your own unit?	93.3	1.5	2.1	3.1	5.2
. . . assigned to a combat unit as a replacement during hostilities?	94.8	0.8	2.1	2.3	4.4
. . . unit engage the enemy in a firefight?	95.4	0.7	0.9	3.0	3.9
. . . a medic, physician, or nurse who had to decide who would receive life-saving care?	93.5	3.2	0.7	2.6	3.3
. . . part of artillery unit which fired on the enemy?	96.2	0.6	1.1	2.1	3.2
. . . unit ambushed or attacked?	95.1	2.1	0.9	1.9	2.8
. . . unit engage in a battle in which it suffered considerable casualties?	96.0	1.4	1.4	1.1	2.6
. . . kill someone or think you killed someone?	97.5	0.4	0.4	1.7	2.1
. . . make any . . . decisions that . . . resulted in death or serious injury to civilians?	98.4	0.4	0.4	0.8	1.3
. . . on a ship or aircraft . . . that was under attack?	98.8	0.1	0.3	0.8	1.1
. . . in an armored vehicle that was under fire?	99.0	0.1	0.1	0.7	0.8
. . . take part in an amphibious invasion?	99.7	0.0	0.2	0.1	0.3
. . . evacuated . . . for battle fatigue?	99.6	0.1	0.0	0.3	0.3
. . . ever a prisoner of war?	99.7	0.0	0.0	0.2	0.3

Note. Similar tables for men and women separately are available from Daniel W. King.

Table 3
Frequencies of Changes in Stressor Endorsements (N = 2,001)

No. of changes	Yes to no		No to yes		Either	
	n	%	*n*	%	*n*	%
0	834	41.7	480	24.0	179	8.9
1	593	29.6	484	24.2	292	14.6
2	330	16.5	427	21.3	412	20.6
3	139	6.9	265	13.2	400	20.0
4	63	3.1	155	7.7	300	15.0
5	24	1.2	84	4.2	184	9.2
6	11	0.5	50	2.5	111	5.5
7	4	0.2	33	1.6	65	3.2
8	3	0.1	9	0.4	28	1.4
9	0	0.0	9	0.4	18	0.9
10	0	0.0	2	0.1	6	0.3
11	0	0.0	0	0.0	3	0.1
12	0	0.0	2	0.1	1	0.0
13	0	0.0	1	0.0	2	0.1

Note. Similar tables for men and women separately are available from Daniel W. King.

was 1.96 (SD = 1.85), and the average number of yes-to-no changes was 1.08 (SD = 1.23).

The correlations between the total number of changes of either type and Time 1 and Time 2 Mississippi Scale scores were .14 and .24, respectively (both significant at $p < .001$). For the number of no-to-yes changes, the correlations with Time 1 and Time 2 Mississippi Scale scores were .08 and .26, respectively (again, both significant at $p < .001$). The yes-to-no change index had a significant .10 ($p < .001$) correlation with Time 1 Mississippi Scale scores, but its correlation with Time 2 Mississippi Scale scores was essentially equal to .00, with rounding.

Cross-Lagged Panel Analysis

In the initial multiple group measurement model, the factor loadings of the four Mississippi Scale scores on each PTSD latent variable (Time 1 and Time 2) were free to vary across genders but constrained to be equivalent over occasions, and the residuals for the PTSD indicators were allowed to covary from Time 1 to Time 2. For this model, $\chi^2(78, N = 2942) = 398.60$, $p < .001$. Then, these PTSD factor loadings were constrained to be equivalent across genders, yielding $\chi^2(81, N = 2942) = 417.65$, $p < .001$. The chi-square difference between the initial model and this more constrained model was significant, $\Delta\chi^2(3, N = 2942) = 19.05$, $p < .001$, indicating that the unconstrained model was the better of the two. As Marsh (1994) pointed out, a minimal requirement for evaluating multigroup structural models of the relationships among latent variables is that their loadings be equivalent in the test of the multigroup measurement model. Otherwise, the interpretations of structural relationships may be ambiguous because, strictly speaking, identical variables with a common factor structure are not present for both groups. Table 4 presents variances, covariances, and correlations among the four latent variables from measurement models estimated separately for men and women. Despite the observation that the intercorrelations among the latent variables are quite similar for men and women, the source of the misfit of the constrained model vis-à-vis the unconstrained model is in the constraining of the unstandardized factor loadings, which define the latent variables. Again, without constraining the loadings—and our data argue that we should not—we have no assurance that the intercorrelations and subsequent structural relationships are among the identical variables for men and women. On the basis of this argument, we estimated subsequent models evaluating the cross-lagged relationships separately for male and female veterans (Bollen, 1989; Horn & McArdle, 1992; McArdle & Cattell, 1994).

The first and most saturated structural model for each gender allowed all structural parameters to be freely estimated. The covariance between the two Time 1 predictors, residualized for previous combat experience, and the covariance between residuals for the two Time 2 outcomes were likewise estimated. For men, the initial structural model produced $\chi^2(36, N = 2702) = 230.82$, $p < .001$. The root mean square error of approximation (RMSEA; Steiger, 1990) was .045, with a 90% confidence interval (CI) of .039 to .050; the associated probability of close fit (<.05) was .94. According to Browne and Cudeck (1993), an RMSEA value less than .05 is indicative of good model–data fit. The comparative fit index (CFI; Bentler, 1990) was .98, and the Tucker-Lewis fit index (TLI; Bentler & Bonett, 1980; Tucker & Lewis, 1973) was .97. Convention has dictated that values of such indices exceeding .90 reflect reasonable model–data fit, and more recent thinking (Hu & Bentler, 1998) has mandated values above .95 as preferred. Two additional indices, the Akaiki information criterion (AIC; Akaiki, 1987) and the Bayesian information criterion (BIC; Muthen & Muthen, 1998), were 44,572.68 and 44,802.85, respectively. The values of these indices carry no meaning in and of themselves;

Table 4
Variances, Covariances, and Intercorrelations Among Latent Variables for Men (n = 2,702) and Women (n = 240)

Variable	Men 1	Men 2	Men 3	Men 4	Women 1	Women 2	Women 3	Women 4
1. Reported stressor exposure, Time 1	**.12**	8.08	.32	.38	**.19**	7.98	.45	.45
2. Reported stressor exposure, Time 2	.73	**.21**	.34	.63	.72	**.28**	.56	1.00
3. PTSD symptom severity, Time 1	.30	.28	**9.62**	.10	.33	.29	**8.10**	.15
4. PTSD symptom severity, Time 2	.27	.38	.66	**12.87**	.28	.47	.64	**14.90**

Note. PTSD = posttraumatic stress disorder. Variances are on the diagonals, covariances are above the diagonals, and correlations are below the diagonals. Critical ratios for all parameter estimates exceed 2.00.

they are used in determining the best of a series of models, with favor given to models with smaller values.

Next, the specification of a model for men in which the two cross-lagged paths were fixed at zero yielded $\chi^2(38, N = 2702) = 263.90, p < .001$; the AIC and BIC were 44,601.76 and 44,820.13, respectively. The fit of this model was significantly worse than that of the more saturated prior model, $\Delta\chi^2(2, N = 2702) = 33.08, p < .001$. Moreover, when the two cross-lagged paths subsequently were constrained to be equivalent, $\chi^2(37, N = 2702) = 244.41$, $p < .001$, this produced a significantly worse fit to the data, $\Delta\chi^2(1, N = 2702) = 13.59$, $p < .001$. In addition, the AIC and BIC were 44,584.27 and 44,808.54, respectively. The significant chi-square differences, together with the larger values for the AIC and BIC, point to the more saturated first structural model as the best representation of the data for men. This model is depicted in Figure 1, which reports standardized parameter estimates and their associated critical ratios. The critical ratio is the value of the unstandardized estimate divided by its standard error. According to Jöreskog and Sörbom (1993), values of 2.00 or higher suggest that a parameter should be retained in the model. As the figure demonstrates, the partial regression coefficient for the cross-lagged effect from Time 1 reported stressor exposure to Time 2 PTSD symptom severity is .08. The partial regression coefficient for the cross-lagged effect from Time 1 PTSD symptom severity to Time 2 reported stressor exposure is .07. The analyses indicated that these rather modest and marginally disparate relationships are nonetheless significant and significantly different from one another: Controlling for the autoregressive effects of Time 1 variables on their Time 2 equivalents and for possible previous combat experience, the effect of Time 1 reported stressor exposure on Time 2 PTSD symptom severity is slightly different from the effect of Time 1 PTSD symptom severity on Time 2 reported stressor exposure. For men, the regression model accounted for 44% of the variance in Time 2 PTSD symptom severity and 53% of the variance in Time 2 reported stressor exposure. Each of these values, with rounding, is almost identical to the square of the correlation between the Time 1–Time 2 comparable latent variables (see Table 4), thus pointing to negligible cross-lagged effects. Yet, deleting (or equating) these cross-lagged path coefficients would result in unacceptable damage to the fit of the model to the data and is contraindicated by the size of their critical ratios as well as the AIC and BIC.

For women, the cross-lagged model with all structural parameters freely estimated resulted in $\chi^2(36, N = 240) = 55.71$, $p < .05$. The AIC was 4,241.89, and the BIC was 4,377.77. The critical ratios for the two cross-lagged coefficients were less than 2.00. We then estimated a simpler model, representing the next logical test in our analysis sequence. The cross-lagged coefficients were constrained to zero to evaluate the significance of these effects or the propositions that (a) PTSD symptom severity at Time 1 was not linked to reported stressor exposure at Time 2, and (b) reported stressor exposure at Time 1 was not linked to PTSD symptom severity at Time 2. When this latter model was compared with the more saturated model, change in the fit to the data was negligible, $\Delta\chi^2(2, N = 240) = 2.60$, $p = .27$. Both the AIC and BIC were smaller; 4,240.49 and 4,369.28, respectively. Hence, the second model, with cross-lagged paths deleted, was judged as providing a more parsimonious fit to the data for female veterans: $\chi^2(38, N = 240) = 58.31, p < .05$; RMSEA = .047 (90% CI = .020 – .070; probability of close fit = .54); CFI = .98, and TLI = .97. The final model for women is presented as Figure 2. Here, 43% of the variance in Time 2 PTSD symptom severity and 53% of

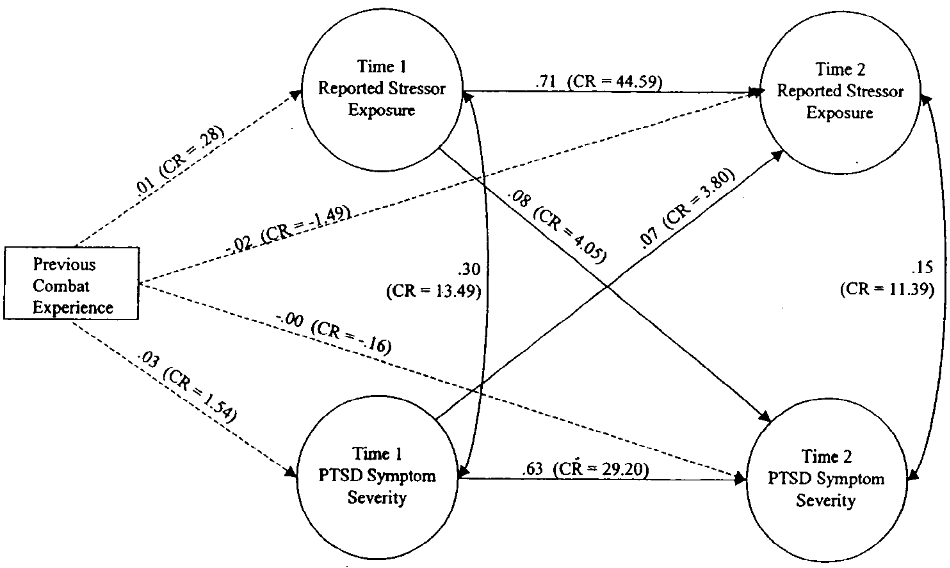

Figure 1. Simplified final structural model for men, showing standardized regression coefficients and their associated critical ratios (CRs). Broken lines represent the effects of the covariate, prior combat experience, on each of the four key latent variables. Solid lines represent associations among these key latent variables. Single-headed arrows indicate directional associations; curved, double-headed arrows depict covariation among variables assessed on the same occasion. PTSD = posttraumatic stress disorder.

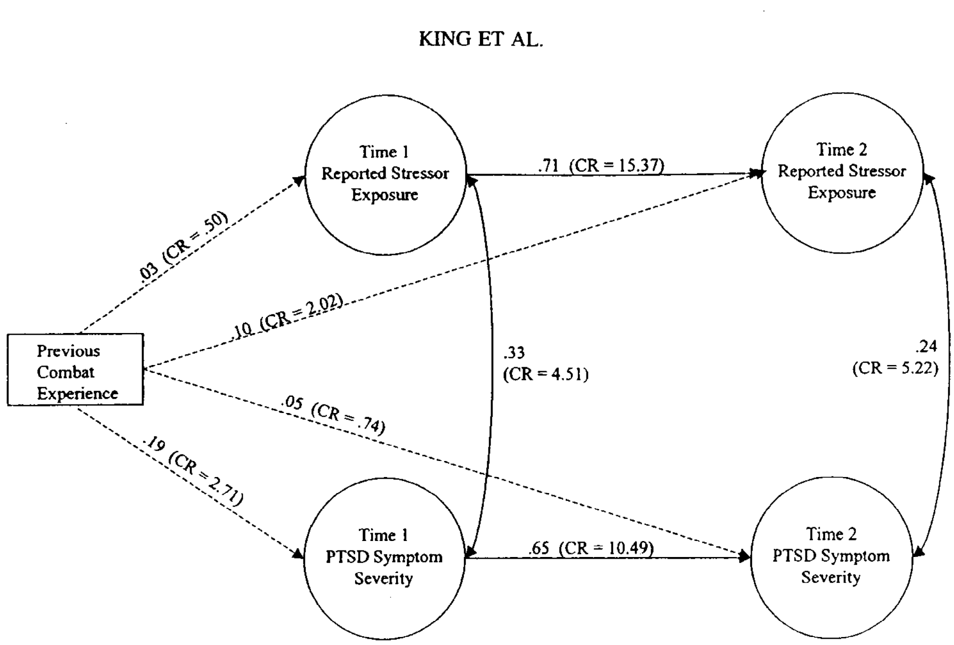

Figure 2. Simplified final structural model for women, showing standardized regression coefficients and their associated critical ratios (CRs). Broken lines represent the effects of the covariate, prior combat experience, on each of the four key latent variables. Solid lines represent associations among these key latent variables. Single-headed arrows indicate directional associations; curved, double-headed arrows depict covariation among variables assessed on the same occasion. PTSD = postraumatic stress disorder.

the variance in Time 2 reported stressor exposure were accounted for.

Discussion

In this study, we profiled changes in retrospective accounts of stressor exposure over an interval of 1 ½ to 2 years in a large sample of Gulf War veterans and documented relationships between these changes and PTSD symptom severity. More important, we sought to clarify the direction of influence in the association between retrospectively reported stressful events and circumstances and symptomatology. To address the directionality issue, we used a cross-lagged analysis of a longitudinal panel design with structural equation modeling techniques, an approach that has been judged as highly suited to this purpose (Bentler & Speckart, 1981; Brown, 1990; Hays, Marshall, Wang, & Sherbourne, 1994).

Our descriptive and bivariate analyses generally corroborated those of Southwick et al. (1997) and Wyshak (1994) in that the responses to exposure items appear vulnerable to some shifts over time (see Tables 2 and 3). As with Southwick et al.'s results, our data indicate that the large majority of the changes were of the no-to-yes type, with persons who initially disavowed a particular exposure subsequently endorsing that event. In addition, the number of such changes had significant, though low, associations with both Time 1 ($r = .08$) and Time 2 ($r = .26$) PTSD symptom severity. The latter correlation is just under that reported by Southwick et al. ($r = .32$).

As shown in Table 2, the range of percentage of change for the items is quite broad, and the large values for some items are indeed puzzling. To offer just one example, the item "see . . . troops killed or wounded" in this study and its counterpart "seeing others killed or wounded" in the Southwick et al. (1997) study were among those with the largest numbers of changes (either yes-to-no or no-to-yes). Fully 25% of all participants in our large sample and 27% of all participants in Southwick et al.'s fairly small sample shifted their endorsements of what would seem to be a presumably explicit, rather extraordinary, and potentially distressing event, denying exposure on one occasion but affirming it on the other. Perhaps the references to "troops" in the present study and "others" in Southwick et al.'s study are sufficiently ambiguous (whose troops? Iraqi or U.S.? what others? enemy or ally?) to elicit different responses from some participants in different contexts many months apart. In other words, it is possible that the observed variations in item endorsement are at least partially attributable to subtleties in the interpretation of wording and language on the two occasions.

On the other hand, the information depicted in Table 3 might argue that the actual number of changes within a 31-item inventory for a typical respondent in this study is relatively unremarkable, given the length of time between assessments. Indeed, some change should be expected. Research on recollections of real-world personal experiences is replete with findings that demonstrate variability in the accuracy of eyewitness accounts as a function of many factors related to encoding and recall (e.g., Kassin, Ellsworth, & Smith, 1989; Loftus, 1979). Additionally, over the specific interval between assessments in this study, there was much media attention to the Gulf War and its aftermath. The public and many of those who fought the war learned details about military logistics for conducting the war, proximity to danger, enemy troop movements, enemy and civilian casualties, and the like. No doubt, this new information was incorporated to some lesser or greater degree into the veterans' perspectives on *their* participation in the war. It would not be surprising, for example, for returned soldiers to learn from media accounts that a previously held belief that they were in a safe zone away from immediate danger was, in fact, inaccurate. Therefore, changes in endorsement of items describing being "close to enemy lines" or "surrounded by the enemy" might be understandable. In addition, many Gulf War veterans participated in postwar military unit meetings, training exercises, or other gatherings where discussion of war-zone experiences might have been another source of knowledge to shape self-reports of stressor exposure. The acquisition of new knowledge, from whatever source, could explain both types of changes in item endorsement: elucidating and clarifying events and circumstances that did occur but were not previously known (accounting for no-to-yes changes) and delimiting details of experiences previously held to be true (accounting for yes-to-no changes).

Relying on the structural equation modeling of the longitudinal panel data, we are able to gain further information on the possible relationship between PTSD symptom severity and retrospectively reported stressor exposure. Despite the rather lengthy interval between assessments, it is noteworthy that the association of each Time 2 latent variable with its Time 1 counterpart is consistently strong. As shown in Table 4, for both men and women, PTSD symptom severity and reports of exposure to war-zone stressors appear to be stable over time. The values for the Time 1 and Time 2 synchronous correlations are also of interest. As Table 4 indicates, these same-occasion associations between reported stressor exposure and PTSD symptom severity ranged from .30 (men, Time 1) to .47 (women, Time 2). These values are generally in line with the cross-sectional literature on trauma and PTSD in veteran samples (e.g., Fontana & Rosenheck, 1999; Litz, King, King, Orsillo, & Friedman, 1997) and in samples drawn from more broadly defined, community-based populations (e.g., Norris & Kaniasty, 1996; Vreven, Gudanowski, King, & King, 1995). However, for men and women, the correlations are higher at Time 2 than they are at Time 1. Although there is no clear explanation for this pattern, it is possible that a potential third variable is influencing the increased reporting of both exposure and symptoms (Table 1) and also the increased covariation between them (Table 4). Once again, we return to the media attention, public concern, and personal worry related to Gulf War illnesses as possible candidates (Fukuda et al., 1998).

Regarding the regression-based coefficients in the model for men (see Figure 1), it seems important to distinguish and consider separately the information gleaned from each of the two cross-lags. First, the regression of Time 2 PTSD symptom severity on Time 1 reported stressor exposure is readily interpretable in terms of a standard cross-lag model seeking to ascertain direction of influence: Does reported stressor exposure predict later PTSD symptomatology, controlling for existing PTSD symptomatology? Indeed, although scores on PTSD remain stable over the Time 1–Time 2 interval, there is an increment in PTSD symptom severity (approximately .60% of the variance) that can be accounted for by initial assessment of exposure. Thus, our data suggest that exposure to highly stressful life events has a lingering and persistent, albeit very small, independent impact on symptom severity across time.

The other cross-lag in the model for men—the regression of Time 2 reported stressor exposure on Time 1 PTSD symptom severity, controlling for Time 1 reported stressor exposure—is more intriguing and germane to this study's purpose, and it appears to need a somewhat different treatment and interpretation. In particular, the autoregressive path from Time 1 exposure to Time 2 exposure does not actually index the stability of veterans' condition or psychological status over time but, rather, indexes recall consistency. The fact that this autoregressive path is large suggests that male veterans' retrospective reports of their war experiences remain relatively congruent from occasion to occasion. Yet the modest cross-lag from Time 1 PTSD symptom severity to Time 2 reported stressor exposure (accounting for approximately .53% of the variance) indicates that male veterans with higher levels of PTSD symptomatology at Time 1 were slightly more apt to show positive recall bias (higher exposure at Time 2 relative to Time 1) than those with lower levels of PTSD symptomatology at Time 1. We caution the reader, however, to recognize the humble effects represented by both cross-lags.

To a very limited extent, therefore, it appears that PTSD symptom severity can influence retrospective reports of exposure to traumatic events, at least for male veterans. We concur with Southwick et al. (1997) that there is a slight tendency toward amplification of traumatic accounts that may be attributed to PTSD symptom severity. It is unclear how this finding should be interpreted, but several possible explanations occur to us. For some veterans, enhanced reporting as a function of symptomatology may be founded in efforts to explain discomfort and distress in terms of prior war experiences; those experiencing psychological problems may seek justification and rationalization by increasing their endorsement of putative causal events and circumstances. Thus, for example, what appears to be a change in the recollection of a seemingly objective and concrete event—"see . . . troops killed or wounded"—may be a consequence of motivated recall. Drawing from the social psychology literature, this tendency might be placed in the context of balance, consistency, or congruity of self-conceptions (e.g., Bachman, 1988; Swann, 1983). Individuals may pursue and recall information that validates negative (or positive) feelings and evaluations of the self.

Moreover, enhanced reporting of prior stressful events could be a consequence of the reexperiencing feature of PTSD, in that ongoing intrusive thoughts and memories promote greater elaboration and reconstruction of forgotten events over the course of time. Future researchers might consider disaggregating PTSD into its primary symptom clusters and then testing whether the severity of reexperiencing symptoms more strongly predicts increases in retrospective endorsements of stressor exposure as compared with the remaining symptom clusters. Along the same lines, one might hypothesize that the level of severity of avoidance and numbing symptoms would have stronger associations with yes-to-no shifts in endorsements. In other words, trauma victims responding with emotional withdrawal and retreat from reminders of prior stressful events and circumstances may be more prone to forgetting, overlooking, or dismissing such experiences (Golding & MacLeod, 1998; Koss, Tromp, & Tharan, 1995). Finally, an alternative theoretical position might postulate an opposite effect: that avoidance and numbing at Time 1 is associated with no-to-yes shifts in endorsement. This hypothesis draws from the notion of a phasic and cyclical presentation of the avoidance and numbing and the intrusiveness PTSD symptom clusters (e.g., Foa, Riggs, & Gershuny, 1995; Horowitz, 1986; McFarlane, 1992), wherein an individual might have trouble remembering important aspects of the trauma at Time 1, but then have some of the details come flooding back prior to Time 2 (thus producing a no-to-yes endorsement pattern).

It is not clear why the cross-lagged relationships were not retained in the final model for women (Figure 2). For this group, all of the accounted-for variance in Time 2 PTSD symptom severity was attributable to Time 1 PTSD symptom severity; likewise, all of the accounted-for variance in Time 2 reported stressor exposure was predicted by Time 1 reported stressor exposure. Without doubt, the failure to support the cross-lagged relationships for women might lie with the smaller sample size and concomitant lack of power to detect such limited effects, especially in light of the very small cross-lagged effects found for men (less than 1% of both dependent variable variances). This finding might also suggest that women are more capable than men in marshaling personal resources and social supports in a posttrauma period essentially to break the links between stressor exposure and symptomatology. Such an interpretation is consistent with recently reported results for cohorts of male and female Vietnam veterans (D. W. King et al., 1999; L. A. King, King, Fairbank, Keane, & Adams, 1998).

In conclusion, we found minimal evidence for psychologically meaningful biases of Time 1 symptomatology on Time 2 reports of stressor exposure. This conclusion is supported by two general findings. First, in women, there was no statistical evidence that individual differences in reporting of stressor exposure was significantly influenced by Time 1 PTSD symptom severity. Second, in men, for whom the association of Time 1 PTSD symptom severity with Time 2 reported stressor exposure was statistically significant, the effect was of such small magnitude as to be considered trivial. Although these findings do not diminish a researcher's need to be cautious when using retrospective data, they do suggest that changes in reporting past events and circumstances over time should not necessarily be a source of undue alarm that Time 1 PTSD symptom severity accounts for those changes. Of course, the findings of this study may be limited in terms of sample and trauma type. It would be beneficial to replicate the analyses across populations other than those exposed to combat and war-zone experiences. Likewise, the generalizability of results would be enhanced by determining if similar outcomes emerge with intervals between stressor and assessment or between multiple assessments that differ from those in this study.

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Received April 12, 1999
Revision received February 23, 2000
Accepted February 25, 2000 ■